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Membership in
The Society for Clinical and
Experimental Hypnosis

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EDITORIAL

The formation of a new scientific society for clinical hypnosis requires the formulation of professional standards and a statement of basic principles. The establishment of such long term policies indicates the nature of the organization as it is reflected in the minds of its officers and the kind of development which it is striving to achieve. It is, therefore, of interest to our membership to call attention to the editorial, "Some Comments on the Postgraduate Teaching of Hypnosis," which appears in the May issue of the Newsletter of the American Society of Clinical Hypnosis.

The editorial begins with a quotation from Dr. Raginsky's presidential address in which he stated, "Teaching of hypnosis for physicians and dentists must always be integrated into the total framework of psychotherapy, and a working knowledge of dynamic psychology and psychiatry is necessary before hypnosis can be used intelligently in the field of the student's competence, be it medical, dental or psychological. While hypnosis may be used alone in experimental situations, it should not and cannot be used as a single method of treatment or diagnosis. It should be used smoothly and flexibly as an integrated part of general diagnosis and therapy."

The unprejudiced student of behavior cannot but accept this informed statement as being the result of sound, clinical judgment and mature, reflective thinking on a major issue. The author of the A.S.C.H. editorial severely criticizes this enlightened formulation as being an "inadequate, sweeping, superficial, dogmatic assertion merely disguised as an intelligently cautious evaluation of presumed threatening possibilities or probabilities." The same writer goes on to state, "One may recall the same type of expectation of calamities that would ensue if psychoanalysis were ever permitted unless certain presumably protective measures were adequately provided." This statement is in direct opposition to the standards of every scientific society in this country and abroad.

The writer of the May editorial also is convinced that if the general practitioner, dentist or anesthesiologist attains the status of

a good technician he does not require "an extensive knowledge of psychodynamic psychiatry." He asks the pertinent question, "Must the practitioner 'in addition to premedical, medical school, internship, residency and specialized training add to it a year or two or more of psychodynamic psychology and psychotherapy?' " In order to understand the significance of this question, we must begin by inquiring what is meant by psychodynamics. The 1952 Conference on Psychiatric Education defines psychodynamics as the "systematized knowledge and theory of human behavior and its motivation. A fundamental postulate of psychodynamics is that most of the significant activities of human beings are motivated and goal directed. . . . While a person may be explicitly aware of some of his motivations, a great deal remains implicit or perhaps quite unconscious, and unconscious motivation may be in direct and severe conflict with conscious motivation. . . . The contributions to contemporary psychodynamics came from several sources. By far the greatest contributions in data, methods and operational principles are those of Freud and psychoanalysis. Another contribution is one made by Adolf Meyer's common sense genetic-dynamic psychiatry. Within recent decades, experimental psychologists have begun seriously to influence psychodynamic developments, especially in the fields of learning theory, gestalt and topological psychology, perception, and personality."

It is necessary for any professional person to learn about the patient's defenses, the manner in which people react to him and to the treatment situation, if he is to become a more effective therapist—whether the method of treatment be psychotherapy or the handling of a patient who comes in "merely" for a simple dental, medical or surgical procedure "under" hypnosis. Whether a doctor is a technician or develops into a student of human behavior will depend upon whether he masters the principles of psychodynamics. The practitioner who is ignorant of the motivational dynamics of human behavior will, sooner or later, hypnotize a severely disturbed, anxiety-ridden patient who presents the external appearance of "perfect calm and complete emotional control" or a prepsychotic patient who comes for the purpose of having a "spell" removed by hypnosis, but only tells the doctor that he suffers from headaches.

The 1955 report of the British Medical Association contains the statement that "hypnotism may involve the rapid or immediate development of a relationship between hypnotist and subject of the same order as is produced more slowly in the course of psychotherapy. . . . The application of the hypnotic technique without such knowledge and experience provides no control of the powerful emotions which may be released." This fundamental statement is based upon the experience of the subcommittee appointed by the Psychological Medicine Group Committee of the British Medical Association. To deny the theoretical and practical significance and clinical ramifications of this statement is to behave like the proverbial ostrich—which not only denies the reality of a situation but offers the best possible target to its enemies.

The psychodynamics involved in the hypnotic relationship have been clearly delineated by Guze, Kline, Mears, Rosen, Schneck, Stillerman, Wolberg and others. They have described the patient's need to dominate or be dominated, the fear of authority which may be experienced as being destructive or dangerous, the fear of losing control of one's will, the patient's wish to prove that he is the stronger, the desire to be humiliated or overpowered, the conviction on the part of the subject that he will fail in the task of hypnosis, the need for dependency, expiation, the feeling of guilt and the terror of intimate interpersonal relations. Hypnosis may mean death, a homosexual relationship or symbolize a rebirth fantasy.

The hypnotist also must know himself, and not derive too much vicarious satisfaction or "power" from the treatment of the patient while in the trance state. He must be aware of the dangers of becoming involved in the patient's paranoid projections, of arousing repressed conflicts which may overwhelm the patient, of becoming the object of the sexual fantasies of the patient, or of removing symptoms in disturbed, unstable individuals who require their symptoms as defensive maneuvers. The patient reacts in the therapeutic situation in keeping with his emotional needs.

To be unaware of these clinical facts is to barge in "where angels fear to tread." Does this mean that every hypnotherapist must be psychoanalyzed? It may come as a surprise to many that it is not recommended that every psychiatrist undertake a personal

psychoanalysis in order to become a competent therapist. I quote from the report of the 1952 conference on psychiatric education whose deliberations "disclosed virtually unanimous agreement" that "it is not necessary to be psychoanalyzed in order to develop competence as a psychiatrist including competence in psychotherapy and psychodynamics." Dr. Karl A. Menninger in the same report emphasized this conclusion by commenting, "I applaud this statement that one does not have to be psychoanalyzed to be a good psychiatrist." The 1952 Report also contains the statement that "for those who are to practice the specialty of psychoanalysis such psychoanalytic training is considered a necessity."

If he does not need to be psychoanalyzed in order to become an effective hypnotherapist, how can the average practitioner become acquainted with the principles of psychodynamics? He can acquire competence by taking courses in psychiatry and psychology on the postgraduate level. He also should associate himself with a group of experienced therapists of all schools so that he can share and profit by their experiences with the scientific disciplines of psychoanalysis, psychology, psychiatry, anesthesiology, psychosomatic medicine and dentistry.

It has been known for more than a century that any person with a grade school education can learn to hypnotize, just as any person of average intelligence can learn to use a hypodermic or to perform certain surgical procedures. The art of medicine, as contrasted to its technical aspect, is not only to learn what procedure to use, but when to use it, how much to do, and when *not* to use this or that technical approach.

The Society for Clinical and Experimental Hypnosis therefore insists that its members learn all that they possibly can about psychodynamics. The A.S.C.H. editorial which is being discussed would lead one to believe that there are no dangers to hypnosis. However, the simplest medical or psychological procedure requires safeguards. The Subcommittee's report of the British Medical Association has this to say, "The dangers of hypnotism have been exaggerated in some quarters. The Subcommittee is convinced, how-

ever, that they do exist, especially when it is used without proper consideration on persons predisposed constitutionally or by the effects of disease to severe psychoneurotic reactions or anti-social behavior. The commission of crimes involving even danger to life is not entirely to be ruled out." The Committee also expressed "strong disapprobation of public exhibitions of hypnotic phenomena."

In the state of Maryland and elsewhere chiropractors and individuals who have purchased spurious degrees in "psychology" are now practicing hypnosis. The Subcommittee's report includes the following statement, "Like other remedies hypnotism has its indications and contraindications, and considerable knowledge and expert judgment are required to decide whether hypnotism is likely to help the patient, and whether it should be used itself or as a complement to other methods. . . . For these reasons the Subcommittee is of the opinion that *hypnotism should not be regarded as a specialty independent of psychological medicine.*" This is the essence of Dr. Raginsky's presidential report.

It is becoming obvious that history is repeating itself, and that if hypnosis is to be given professional status it must be saved from those who will not take the time and effort to understand its dynamic implications and who reject all of the progress that has been made in psychiatry and psychology during the past 50 years. One of the severest critics of The Society for Clinical and Experimental Hypnosis has described it as being a "society of professors." *It is just that.* It has the ideals of a university community, the high standards of scholarship, and the devotion to a cause which characterizes the sincere student.

The era of the "three day wonder" who knows it all because he has attended a three day seminar in hypnosis is over. It was only a transitional period which helped to emphasize the immanent need for medical schools and university departments of psychology to include postgraduate training in hypnosis in their curricula.

There are now two distinct societies. One is a grass roots association which is devoted to techniques, which can be readily

and quickly acquired, a society which any professional person may join. The other is a carefully screened group of serious investigators and experienced clinicians who have determined to maintain high standards of practice and to add to what we already know about hypnosis.

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LEARNING UNDER HYPNOSIS: WHAT DO WE KNOW? WHAT SHOULD WE KNOW?

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What Do We Know About Learning Under Hypnosis? What Should We Know?

Exactly one unequivocal experiment on the learning of new material under hypnosis has been conducted in the entire history of hypnosis, and this experiment, by Cooper and Rodgin(3), tested only one subject and was interested in the effects not of hypnosis itself but of the time condensation method employed.

Weitzenhoffer concludes from Cooper's experiments that "under these conditions (time condensation) we should expect an increase in learning, simply because of the longer time allowed for this, and not because of any special facilitation of the learning process itself. . . . There is reason to believe . . . the absence in improvement in learning nonsense syllables does extend to the case of learning in hypnosis. Thus it is probable that temporal condensation was the cause of the observed increase in recall"(26, pp. 168-169). That is, not hypnosis but Cooper's special technique gave Cooper's results.

A careful review of the evidence forming the basis for an evaluation of our present knowledge of the effects of hypnosis on learning seems to lead to a conclusion almost diametric to that commonly believed: first, there is little if any conclusive experimental evidence treating this question, but second, what evidence there is, and the overwhelming impression gained from clinical observation, indicates quite definite and possibly striking improvements in learning done while under a well-managed hypnotic trance.

The layman thinks of hypnosis as a method that could teach him the speech he has to memorize, a new language, a part in a play, material for an examination; but because it has apparently been controversive as a psychotherapeutic tool he assumes it to be

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of limited value for this other type of problem. More important and more dangerous matters are also involved—schoolroom education or company training on the one hand; superficial symptom removing—smoking, obesity, etc.—on the other—whose principles would derive directly from an understanding of the learning process under hypnosis, and whose dangers would best be studied, as are the side-effects of pharmacological agents, in the situation itself. And if hypnosis can be used to influence learning its use might lead to major advances in psychological research.

A fair amount of study has gone into the question of hypnotic recall of previously learned experiences; this is obviously the question that the backward-looking Freudian or Pavlovian oriented psychotherapist would ask. But practically nothing is known about training people under hypnosis—a question that is probably completely divorced from problems of emotional complications or even relevance, and yet is of major practical and theoretical consequence.

Review

Cooper's one experiment that is relevant to the problem of learning under hypnosis was part of a series conducted with several colleagues and finally summarized in his monograph on Time Distortion(2). Cooper was concerned primarily with the manifestations and effects of this particular phenomenon—the greatly speeded subjective time perception of a subject who has been trained not only to deep hypnosis but also in time condensation techniques. Whereas Cooper concludes that his results indicate the effectiveness of time distortion under hypnosis for learning, there is little reason to believe (certainly no reason from the design of his experiment) that effective utilization of the hypnotic trance was not the sufficient cause of his results. And even if time-distortion is a necessary feature of the effective hypnotic trance, it is nonsense to think of this as anything other than hypnosis: how else do you distort time?

Cooper studied one subject over a series of 30 matched trials of learning 5 pairs of 3-letter groups presented in writing. His experimental design appears to have done a fine job of eliminating any extraneous bias. His results are striking: whereas an average

of 31 seconds per pair was needed to learn during the waking state, only 7 seconds was needed while under hypnosis.

This experiment has been criticized as being inconclusive because only one subject was employed and no statistical comparisons were made. Actually, the situation is quite the reverse: the results are so striking and overwhelming that there is no need to use a statistical microscope when old-fashioned eyes suffice. One subject means that there are certain questions as to generalizability. But certainly it would be far harder to stomach the suggestion that Cooper's subject was the unique learner in the whole universe, rather than that there are probably at least *some* few people like him. And that is all we would expect to conclude from an experiment on effects of a method that cannot and would not be universally applied in any case. It was not because Rhine reported striking ESP runs for only an occasional subject that his reports were discounted, but rather because the subject never happened to show again when someone else happened to be around.

Of course we do not know from Cooper's experiment how common a phenomenon he has pointed out, but that is a problem for other, and different kinds of, studies. The only possible weakness in the use of one subject might lie in the subject's motivation toward the experiment's succeeding and his biasing the results. An easy check could be made by comparable training of well-motivated subjects in the waking state with the same material. I attempted to do this myself, because I was skeptical as to both this point and the method of training employed awake, and taught myself a list of 5 pairs of 3-letter groups. Learning took 26 seconds per pair, results quite similar to Cooper's. This answers another possible criticism of the study—that the waking learning was overcontrolled, so that the subject had to use the methods (repeated writing-down and associating of the letter groups) the experimenter prescribed. This could have led to inefficient learning simply because of experimental overconscientiousness, but apparently it didn't.

In fact the one methodological criticism that might justly be made of Cooper's experiment is that he was inelegant in doing too much. He ran 30 learning sessions in each condition, when, because of his extremely conclusive results, he needed far less.

Cooper measured recall and relearning 24 hours after each original session and here found a smaller but appreciable gain for learning done under hypnosis—on the order of 15-25%. This again has been misinterpreted as indicating that somehow the extra learning is only temporary. On the contrary: the learning was almost 450% faster under hypnosis, but there is no reason to expect that material learned while awake was not as completely learned as material learned in the trance. In reality here is a hint of a second striking phenomenon—an appreciably flatter forgetting curve for material learned in the trance (though the subject's motivation contaminating the results could quite justly be raised in question here).

This is precisely the confusion in a study by Gray(9), at least as analyzed and reported in his publication. Gray trained his subjects to complete learning but does not report whether there were any differences in training time—the crucial question. Then he compared recall of material learned in trance with recall of material learned awake. Again, he reports very slight (2 or 3%, no doubt insignificant) gains for hypnosis. He concludes that hypnosis has no effects, but he has studied only the second phenomenon —the permanence of learning. In any case, this experiment should be questioned because of the peculiar design—six school children with spelling problems were used as subjects and trained to spell words they had just finished misspelling. Personality and intellectual factors are thus involved in performance, and the waking practice of pre-experiment spelling sessions, even though the words may have been spelled wrong (but no doubt corrected by the experimenter before he moved on), might well have led to a greater redintegrative effect during the waking than during the trance learning trials.

Young performed several experiments in which his subjects learned material while in the trance state(27). He classified his subjects as 6 in light, 6 in deep and 4 in somnambulistic trances. For nonsense syllable learning, he found a 15% improvement under trance for his somnambulistic subjects, and a 9% to 15% decrement for the other groups. For adjective-noun learning, he found a 9% improvement for the somnambulist group, a 2% im-

provement for the light trance group, and a 14% decrement for the deep trance group. He further found that this material was retained 16% better by the somnambulist group, 1% better by the light trance, and 5% worse by the deep trance group. Young also employed 6 "control" subjects—people who were not hypnotizable and were used in the waking state but given the same suggestions given subjects in the trance (Young does not specify what these suggestions were, nor does he lead the reader to believe that they were standardized across subjects to the extent that they could be equated in this way). These 6 subjects showed a 2% deficit for nonsense syllable learning, a 4% deficit for adjective-noun learning, and a 7% improvement in adjective-noun retention.

Young conducted two additional learning-related experiments. He found a 4% decrement in memory span for names in an unspecified group of 9 subjects; and he found negligible changes in learning a list of 20 words in logical sequence in an unspecified group of 11 hypnotized and a group of 3 control subjects.

Young's overall thesis and conclusions stress the absence of improvement in performance under hypnosis and the autosuggestion quality of the trance state as he understands it(28, 29). As a consequence, hints of trance effects, such as the 9% to 16% improvements summarized above, are usually neglected in the review literature. But once more there is the hint of at least some improvement under trance—with the added indication that this will be so only when conditions are favorable—as here, when subjects have been trained to the somnambulist state.

Strickler conducted the experiment(25) whose results led Dorcus to conclude that hypnotism had been shown to have no heightening effect on learning, except possibly in the early stages. But this experiment was designed to measure the effects of post-hypnotic amnesia, and only in passing does Strickler present the data that he collected on his subjects' learning curves.

Again, this experiment should be looked at in detail so that it can be evaluated properly. I will attack it as strongly as possible; I am expounding an opposing thesis. It deserves better treatment and to a certain extent does oppose Cooper's findings—a happy

situation for some crucial experimentation. But certainly it is far more vulnerable to criticism than Cooper's work.

First, Strickler chose his 4 subjects because, of a large group he had hypnotized satisfactorily, they were among the few who exhibited complete posthypnotic amnesia. That is, they regularly remembered nothing of what had happened while under hypnosis. If there are individual differences in effectiveness of learning under hypnosis one of the variables rather likely to be relevant would be how well the subject remembers from trance to waking. If Cooper may have happened to choose an unusually good subject, Strickler may have tried to choose four unusually bad ones.

Strickler was interested in the amnesia, and reports almost total amnesia for the learned material 15 minutes after learning in the trance state. He used a drum designed by Hull that presented symbols the subject viewed through an aperture while the experimenter said the syllable to be associated. If you are looking for flaws, it's not unreasonable to suggest that the double involvement in two different perceptual processes might make effects of any perceptual dulling or hypnotic withdrawal especially great. It is interesting to compare this technique with the one used by Cooper, who had his subject hallucinate that he was writing his lessons when in fact he was sitting motionless, eyes closed. Finally, Strickler may have got results anyway. Hull reports(11, p. 140) that subjects received 36% more learning under hypnosis, but Strickler's presentation of his own data does not appear to confirm this. In his first few learning trials learning was 33% better under hypnosis, but after that awake learning caught up. (There is no way to make a statistical evaluation of this phenomenon from the data Strickler reports in his article, but it is quite likely that the differences on the first few trials are significant.) This phenomenon occurred for all subjects on all trials. In other words, it seems to be inexplicable but there. For a short time subjects learned faster under hypnosis, then they learned faster while awake, catching up, and then they learned at the same rate. A possible explanation may come from the suggestion method: Strickler told his subjects once, at the beginning of each trance session, that they would learn easily while in the trance.

Possibly we are seeing the effect of this suggestion and would continue to see it if the suggestion had been repeatedly given.

Coors(5), in an unpublished experiment reported by Hull(11), paralleled Strickler's experiment with stylus maze learning as the dependent variable. Her results were essentially the same as Strickler's—there was an average excess loss from learning under hypnosis of about 45%. There is no report in Hull's summary of this experiment as to the relative time of learning in the two states.

Life(15) and Patten(21, 22) conducted two experiments in Hull's laboratory that indicated that practice under hypnosis was not lost, but rather resulted in regular increases in the learning curve that could not be differentiated from waking practice. Thus, when the suggestion of post-hypnotic amnesia was not made in Hull's laboratory, its effects were not felt. Hull specifically makes the point that this amnesia appears to be the result of suggestion, and not a primitive tendency. Glasner concludes, with what appears to be justice, that the results "merely demonstrate the relative effectiveness of this suggestion. And it may well be that the opposite suggestion, of improved recall, would be equally effective"(12, p. 57).

Experiments by Mitchell(18) and Nagge(19) might have provided some answers to the question of trance effects on learning, but the relevant data, although collected, was never published. These experiments were designed to test the effects of interpolated learning on remembering. In both experiments, there were runs in which subjects learned while under hypnosis (and recalled and relearned in a subsequent trance state). The experiments then examined the effects of interpolated learning in the same or in different states (with opposing results). Nagge obtained approximately 50% better results on retention in trance of material learned in trance, no matter what was interposed, in comparison with retention awake of material learned awake. Mitchell got much less conclusive, generally null results.

Two recent experiments have tested the effects of post-hypnotic suggestion on tasks related to learning. These, then, are related to the central problem examined in this paper—the effects of the

hypnotic trance on learning—but their results must to some extent be considered only inferential, giving indications of what might be the case for the hypnotic state. Hammer tested the effects of post-hypnotic suggestion on the performance of 9 subjects on 25 behavioral tests(10): 5 tests were affected beyond the 1% level, and an additional 13 beyond the 5% level of probability. Among the tests related to learning, digit span forward, digit span backward, and speed of reading comprehension were all improved at the 5% level. The two tests used that were primarily of learning were also improved—of nonsense syllables at the 10% level, of meaningful symbols at the 1% level. The magnitude of improvement was over 33% for nonsense syllables and almost 50% for meaningful syllables. The criticism might be made that motivational changes might account for the results, and should have been controlled, but the improvement in performance is great enough to at least make us wonder.

Sears tested the effects of post-hypnotic suggestion on recall of objects seen on a tabletop for 30 seconds(22). He found 10% greater recall under post-hypnotic suggestion, but he does not make a statistical evaluation of this aspect of his data, and does not present the data in a form that allows the reader to do so.

Sears(23) found statistically significant but small improvements in learning to decode Morse code messages. Only after 30 hours of practice, however, did his results become significant (at the 5% and 1% level, depending on the speed at which the code was sent). He used two groups matched for intelligence, of 25 subjects. His results might have been much more conclusive if he had used subjects as their own controls. More important, Sears mentions that decoding is a boring task and that in fact the group that worked while awake found it drudgery (subjects were paid $1 per hour). Eleven awake and six hypnotized subjects dropped out (it seems likely that the hypnotized subjects had quite different reasons for discontinuing). Sears reports that the hypnotized group, though not interested in decoding, reamined interested in the experiment, were highly motivated, and felt less strained by the boring sessions (an interesting hint that hypnotic learning might have side-effect

advantages, not disadvantages). This throws the motivational factor right into the middle of the picture; certainly a skeptic would be justified in discounting this experiment on the basis of the motivational differences that arose between the two groups.

A later article by Sears(24) reports a study he conducted earlier than the one just discussed in which striking but unevaluatable results were obtained. Eleven students with IQ's over 135 but C averages in school were given special remedial learning sessions under hypnosis. The following semester, four got A's, three got B's, three got C's but higher C's than before, and one Sears does not discuss because he had a "basic personality problem." Certainly these are better results than are claimed for the various schools of psychotherapy and families of pharmacological agents, but open to the same criticisms. There is no telling how much the more conventional methods of remedial training would have helped (Sears reports that he used hypnosis after having tried conventional methods and seen that they weren't working, but it's doubtful that he collected any next semester grades or other objective criterion measure); students probably got a bang out of the experiment and might have adjusted better in general (and tried harder); teachers might have been interested in the experiment, and grades aren't as objective a criterion as all that. Still, as a clinical report here is another slight indication in favor of heightened learning under hypnosis. And our understanding of the possible dangers might be increased by more information on the one person whom Sears has transformed in his report from an experimental subject into a human being.

A similar clinical study was conducted by McCord, who gave special practice in a wide variety of materials to a boy with an IQ of 55(16). Both precise measures of retention and teachers' reports of improvement appear to indicate the effectiveness of the technique, but once more there were no controls.

Glasner reports a completely uncontrolled observation that is probably typical of the experience of a number of hypnotists, and is the sort of clinical indication of striking effects that cry for

experimental tests. He taught one subject a Hebrew-English word-list. He reports: "a single oral presentation resulted in an amazingly high percentage of post-hypnotic recall"(12, p. 59). Similar instances were reported to me when I asked around; many hypnotists may have had this sort of experience.

Cooper conducted two additional partially relevant studies along with several co-workers; both of these studies however resulted in either inconclusive or non-evaluating findings. Cooper and Erickson(1) had subjects review for a history examination while in a trance, with inconclusive results. One of their subjects did hallucinatory violin practice, with marked subsequent improvement in her performance, according to her own and her husband's reports. Cooper and Tuthill(4) experimented on improvement in motor learning (subordinate hand writing), but their controls were inadequate.

Mischenko(17) reports that reflexes conditioned under hypnosis were learned more rapidly. Thus, for more unconscious learning, we again have an indication of facilitation by hypnosis. Mischenko reports, however, that there were no differences between trance and awake conditioning of discriminations or inhibitions. This experiment, along with experiments conducted by Leuba(13, 14), further indicates, for this type of learning, the effect of facilitation of learning from one state to another already reported by Life and Patten.

A great deal of controversy has arisen over the relations between conditioning and the hypnotic state, and over the effects of hypnosis on conditioning. Since this takes us beyond the problem of specific effects of hypnosis on rote learning, there will be no attempt to go further into the situation here. It should merely be noted that there is at least some reason to believe, on the basis of experiments on conditioning under the influence of hypnosis, that there may be some facilitation due to the trance state.

There is one further type of related experiment that should be mentioned in passing—the experiment in which traditional learning does not take place, but rather mood, attitude, or symptom

change is induced under the hypnotic trance. For example, a fine experiment by Dickson(6) indicates that post-hypnotic suggestions as to emotional mood will affect problem-solving behavior. Glasner (8) reports changes in attitudes resulting from hypnotic suggestion greater than changes resulting from waking suggestion. Many more factors are operating in this type of experiment, but once more there is an indication that something is going on.

Conclusions

This is the sum of relevant experiments, and the vast majority are inconclusive or of only tangential interest. Cooper found a 450% increase in learning under the special time-condensed hypnotic state for a single subject. Early experiments by Gray and Strickler, which are usually cited as the basis for the conclusion that the hypnotic state does *not* enhance learning are open to a number of methodological criticisms and, in fact, give certain indications of a small effect. Young's experiments sum to conflicting results—in some cases hypnosis appears to have facilitated learning, in other cases to have had no effect. Nagge's and Mitchell's opposing results sum to much the same confusion. Most important, none of the above experiments was designed specifically to test the effects of the trance state on learning, and in most cases the data pertinent to this point is not presented or fully analyzed. Sears' experiment on learning Morse code was designed to test effects on learning and found statistically significant positive effects, but it is especially open to criticism on the grounds of contaminating motivational influences. Other experiments test only related effects: post-hypnotic suggestion, conditioning, transfer of learning. And the other evidence is either inconclusive, as in the case of Cooper and Tuthill's experiment, or of merely clinical interest, as some of Sears', McCord's, and Glasner's observations.

What conclusions can be drawn from these experiments? If we accept Cooper's results (and only the present author has questioned them), then we have a striking indication that something is happening, though we don't know whether it is due to increased learning because of a favorable trance state, because of the specific

time condensation technique used, because of the peculiarities of the particular subject employed, or possibly because of better utilization of mnemonic devices while in a trance state.

The other experiments indicate a general trend of positive results—increased learning while under trance—but improvement on the order of 2% to 40%; not 450% as in Cooper's experiment. The exceptions are the experiments conducted in Hull's laboratory on the effects of post-hypnotic amnesia, in which a conflicting influence, the amnesic state, was deliberately induced.

These results would certainly seem to indicate that until new experiments, in which benign conditions for learning under trance are maximized, are conducted, there is little justification for concluding anything, much less that the trance state cannot improve learning. Further, a number of aspects of the hypnotic trance—depth, amnesia, type of suggestion, etc.—appear to be crucial. These should be manipulated until the most favorable, not the most unfavorable, conditions are found. And the important questions as to which of these are legitimate qualities of hypnosis and which contaminating variables should be both raised and, to some extent, held in abeyance for the time. "Suggestion" or "motivation" or "attention" may well prove to be the crucial variable, the hypnotic state merely the condition that allows the full force of the operating factor to be felt. But if the operating variable can have its maximal effect only under hypnosis, or if hypnosis helps us to understand and manipulate the total situation, experiments in this area will still have been of major importance.

Suggestions

One of the strangest aspects of our lack of knowledge as to the effects of hypnosis on learning is the comparative ease with which crucial experiments could be performed. Presentation of matched lists to the same subject, one under awake, one under trance conditions, is virtually all that is required. For example, Glasner could rather easily have gotten control runs while the subject whom he had taught Hebrew was awake. If two subjects

were used, one learning awake what the other learned in the trance, even the matching of material would not be critical. Young suggests that his use of control subjects is an important advance (one that was never taken up by later experimenters), but it would seem more reasonable to use control *methods* on the experimental subjects—simulating the trance suggestions in the awake control trials, and, even further, heightening the suggestions and motivational appeals in the awake trials. This would have the effect of minimizing, by simulating, any hidden motivational influences while in the trance. For example, subjects could be given money, or taught material that would help them in exams. Finally, a range of learning materials could very easily be used—meaningless nonsense syllables through language lists to factual prose passages to more abstract ideas; unimportant to ego-involved materials; low anxiety to high anxiety materials, etc. And the range of hypnotic states could also be assessed and specified. A simple learning battery might easily be used from the very beginning in working with a new hypnotic subject, the first few trance inductions actually serving as pre-tests of battery, type of trance, and subject.

We know a good bit about many of the variables involved in the learning of material under hypnosis, and a number of interesting and testable hypotheses could be generated. We might hypothesize greater increases in learning under hypnosis the greater the meaningfulness and ego-involvement of the material, decreases in learning the greater the productive anxiety, but occasional dramatic increases when anxiety that had led to learning blocks has been broken into by hypnotic procedures. We certainly want to manipulate the quality of the trance state—to see whether time condensation is in fact crucial for heightened learning, to specify the optimal hypnotic state. We would be able to learn an enormous amount about the hypnotic state once we have a dependent variable sensitive enough to its changes so that we can look at its differential effects, begin to see what it is about.

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HYPNOTIC INDUCTION BY USE OF NON-MEANINGFUL LANGUAGES: A PILOT STUDY[1]

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Problem

The question has often been asked as to whether hypnosis is due to suggestion, rhythm, monotony, or some other as yet unidentified factor. In an endeavor to answer this question a regular hypnotic induction based on the use of a spiral disk was translated into three different languages: Bohemian, Japanese, and Spanish. Three female operators, each speaking her native language, were instructed in the use of hypnotic techniques and in proper voice quality. Tape recordings were made of each language.

Method

The subjects were 46 University of Denver students. Of these there were 19 women and 27 men. All subjects were volunteers.

All subjects were asked to listen to the tape recordings. They were all given the same instructions.

The subject was seated on a couch and given the following instructions: "We would like to have you sit back in a comfortable position and listen to this tape. When the tape is finished, we would like for you to tell us which of the three parts is most relaxing. You may close your eyes if you wish, or you may look at the opposite wall. There is to be no talking during this time." The combined recording of the three languages lasted approximately 18 minutes. Six composite sets of recordings were made; the language sequence consisted of the six possible combinations (1-BJS, 2-BSJ, 3-JBS, 4-JSB, 5-SBJ, 6-SJB). This was done in order to eliminate any influence of sequence or order of the languages. The tapes were then used in order as the subjects volunteered. Thus the first four tapes were heard by eight subjects and tapes 5 and 6, by seven subjects.

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[1] This study was conducted under the research program of the Psychology Department of the University of Denver.

Report of Results

Of the 272 subjects that were screened before we started to use the "relaxation tapes," 140 were hypnotized to a medium trance state in a 50 minute period. Of the 46 subjects who had listened to the tapes, 41 were later hypnotized to a medium trance state in a 50 minute period. The lapsed time between the "relaxing" and "hypnotic" sessions varied from one to seven days.

The report of the subjects was that the following languages were the most relaxing:

Spanish 26
Bohemian 17
Japanese 3

As reported by the subjects in order of preference, the following were least relaxing:

Japanese 28
Spanish 9
Bohemian 9

The question is raised as to whether these subjects were later more susceptible because of the boredom, the similarity in the voice qualities, the familiar surroundings, the previous establishment of rapport, or because they had become accustomed to the situation.

Of the 46 subjects, 14 went into an apparent light trance, or deeper, while listening to the tapes (Le Cron-Bordeaux Scale). Criteria used to determine trance depth is as follows:

A. Physical Appearances observed by the experimenter:

1. The subject seemed to be asleep. His head was nodding, and he awakened with a start.

2. The subject was breathing with each count (in the foreign language) and seemed very relaxed.

3. The experimenter had to count to awaken the subject.

4. The subject did not hear the telephone ring and denied hearing it when questioned about it.

5. The subject seemed to be asleep; his breathing was slow and regular.

B. Experiences reported by the subject:

1. I saw a figure of a man and a round yellow fringe that

looked like the sun.

2. I felt that I was dissociated.

3. My arms felt like lead.

4. I felt semi-conscious; I did not hear the last part of the tape. The spiral did everything that the record said, although the spiral did not move. (Subject spoke Spanish and the spiral disk was on the back side of the desk).

5. The voices seemed to fade away and then grow louder.

6. I was completely relaxed. (This was also apparent to the observer.)

7. I felt tired and heavy; I felt like sleeping.

8. I could have gone to sleep.

9. I was almost asleep.

The 14 subjects were judged to be in a light trance, or deeper, by the observed behavior and by the reported feelings and experiences during the session.

Of the 14 subjects that were in an apparent light trance 13 were later hypnotized to a medium trance (Davis and Husband Scale) in a 50 minute period. The other one was hypnotized to a light trance in the same amount of time.

Summary

Forty-six University of Denver students were asked to report on which of three foreign languages was most relaxing. Fourteen of the subjects went into apparent light trance while listening to the tapes. All 14 of these subjects were later found to be hypnotizable, 13 of them to a medium trance state in a 50 minute period. The proportion of this group that were later hypnotizable was considerably higher than was found among the subjects who had not listened to the tapes. (41-5; 140-132) Since this is only a pilot study it should not be considered as giving any more than indications of future study. Future work should be directed so as to explore the possibilities of accentuating monotony, rhythm, or actual similarity in the different languages. At the present time we are unable to say which of the above may be the reason for the increased susceptibility or whether there may be other factors.

AN EXPERIMENTAL EXAMINATION OF THE DISSOCIATION HYPOTHESIS IN HYPNOSIS

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Problem

Dissociation hypothesis in hypnosis is one of the most interesting but ignored theories of hypnosis proposed in the past. Dissociation hypothesis was originally adopted by Janet(4) in the domain of psychopathology, in order to account for the hysterical symptoms, and later was supported and extended by some psychologists studying hypnosis: Prince(11), Burnett(1), McDougall(5) and Sidis (12).

According to the dissociation hypothesis in hypnosis, there would be the emergence of a secondary consciousness in hypnosis, that is, the appearance of a group of unconscious memories and activities which temporarily occupy the stream of consciousness. In other word, hypnosis and waking state are two completely independent functions.

Messerschmidt(6) measured the amount of interference between conscious and unconscious task which were performed simultaneously, and came to the conclusion that both functions of hypnosis and waking state are not completely independent.

Mitchell(8) assumed that if the dissociation hypothesis was valid, "when the interpolated learning is done in a different state from the original learning, the retroactive inhibition effects of the interpolated learning would disappear." She made an experiment with two subjects and found that the interpolated learning in hypnosis, on the contrary to the above assumption, had yet some retroactive inhibition effects on the original learning in waking state and vice versa. Then, she concluded that waking state and hypnosis are "not completely dissociated, although there is an apparent amnesia."

Hull(3) said, "The results of Messerschmid and Mitchell . . ., suggest rather strongly that the whole concept of dissociation as functional independence is an error," and hold a negative opinion

on the problem.

However, after two years, Nagge(9) pointed out two defects of Mitchell's experimental design: that is, (1) it had a small number of subjects, and (2) post-hypnotic amnesia suggestions were given to her subjects. He repeated the same experiment, using ten subjects who showed spontaneous post-hypnotic amnesia in the absence of the suggestion. And he found that the retroactive inhibition effects of the interpolated learning were appreciably decreased when the latter was done in hypnosis and the original learning in waking state, and vice versa, the saving score amounting to the gain of the same degree as what was attained when the interpolated learning was not done, but the rest was given, and the saving score was greater when the rest was given in hypnosis. These results, contrary to Mitchell's findings, supported the dissociation hypothesis in hypnosis.

In this article, an experimental examination of the dissociation hypothesis in hypnosis will be shown, using the well-known phenomenon of retroactive inhibition, and following the previous experimental studies.

Method

The experiments of Mtchell and Nagge have some differences, that is, (1) the number of subject, (2) presence of post-hypnotic amnesia suggestion, (3) hypnotic induction time, and (4) temporal position of relearning activities; the following experimental design was taken with the above points in mind.

The material for memorization in this experiment was lists of 24 sets of nonsense syllables constructed of Japanese Kana (cursive character), one of which contained six syllables. The anticipation method was used by presenting the material serially. Exposure time was 2 seconds for each nonsense syllable. Since each list has six nonsense syllables on the white cards, there were six exposures to one revolution of this list. There was no interval elapsed between trials. Each list was presented repeatedly in series to 2 successive repetitions without error. The general procedure was that of a retroactive inhibition experiment. Every subject learned two lists of nonsense syllables during the daily sessions. A ten

minute interval was interpolated between the original and interpolated learning. The second material was learned in the same manner. After 25 minutes of the completion of the original learning, the first material was relearned. A half of the interval between each learning activity (original, interpolated, and relearned) was spent for hypnotic induction or dehypnotization.

The original, interpolated and relearning activities were done under the following conditions.

The sequences in the work reported here may be divided into four groups; control sequences, dissociated sequences I, dissociated sequences II, and rest sequences.

If the dissociation hypothesis in hypnosis is adequate, dissociated sequences I would not yield any retroactive inhibition, but yield a greater saving score than that of control sequences consisting of waking and hypnotic control sequence.

In dissociated sequences II, the relearning activities are done in a different state from the original learning. Therefore the effects of dissociation of interpolated learning and the effects of relearning in a different state from the original would be evident in the light of the results in these sequences.

Furthermore, the results of rest sequences (waking rest and hypnotic rest sequence) would lead us to find the degree of completeness of dissociation, because rest sequences consist of original learning and relearning of the first material only, and have no interpolated learning of the second material. In other words, the procedure of rest sequences used here is that of control sequence in the retroactive inhibition experiment in general.

Subjects were seven boys (12-13 years of age) from Yonezawa Middle School in Chino, Nagano, who showed complete hypnotic amnesia and complete spontaneous post-hypnotic amnesia without definite suggestion.

Table 2 shows a greater saving in trials for dissociated sequence A (W-T-W) than waking control sequence A (W-W-W). All subjects, except one, show a greater saving in relearning, therefore the decrement of retroactive inhibition in dissociated sequence I-A. There is a mean saving of 53.9% for dissociated sequence I-A and only 26.7% for waking control sequence.

TABLE 1

SEQUENCES OF THE STATES DURING LEARNING

SEQUENCE		STATE DURING LEARNING		
		Original Learning	Interpolated Learning	Relearning
Control	A	Waking	Waking	Waking
	B	Trance	Trance	Trance
Dissociated I	A	Waking	Trance	Waking
	B	Trance	Waking	Trance
Dissociated II	A	Waking	Trance	Trance
	B	Waking	Waking	Trance
	C	Trance	Waking	Waking
	D	Trance	Trance	Waking
Rest	A	Waking	Waking Rest	Waking
	B	Waking	Trance Rest	Waking

Results

Experimental results are shown in terms of saving score of trials and errors.

TABLE 2

PERCENTAGE OF SAVING IN TRIALS

SUBJECT	SEQUENCE			
	Control		Dissociated I	
	A	B	A	B
K. I.	17.6	44.0	57.1	13.6
K. Y.	—36.4	44.4	54.6	28.6
H. M.	50.5	.....	50.5	.....
E. T.	43.5	.....	59.1	.....
A. Y.	38.5	.....	48.2	.....
Mean	26.7	44.2	53.9	21.1

TABLE 3

PERCENTAGE OF SAVING IN ERRORS

SUBJECT	SEQUENCE			
	Control		Dissociated I	
	A	B	A	B
K. I.	—33.3	24.6	77.9	—11.3
N. Y.	—29.1	42.0	63.8	4.8
H. M.	42.0	.....	25.0	.....
E. T.	20.0	.....	42.1	.....
A. Y.	26.3	.....	68.0	.....
Mean	26.0	33.3	55.4	3.3

Table 3 shows the same tendency. These results may verify Nagge's findings and support the dissociation hypothesis in hypnosis.

However, a comparison of the scores of dissociated sequence I-B (T-W-T) and that of hypnotic control sequence (T-T-T) shows that the latter has a greater saving, therefore less retroactive inhibition than the former. It seems here that these results may be negative for the above hypothesis.

Although dissociated II-A sequence (W-T-T) contains the same sets of state during learning except relearning, the results show smaller saving. On the other hand, dissociated II-C sequence corresponding dissociated I-B shows a greater saving. (Tables 4 and 5).

TABLE 4

PERCENTAGE OF SAVING IN TRIALS

SUBJECT	SEQUENCE—dissociated II			
	A	B	C	D
K. I.	6.7	—5.3	42.9	28.5
N. Y.	6.3	33.3	47.1	78.6
Mean	6.5	14.0	45.0	58.6

TABLE 5

PERCENTAGE OF SAVING IN ERRORS

SUBJECT	SEQUENCE—dissociated II			
	A	B	C	D
K. I.	—48.8	—56.4	44.4	23.9
N. Y.	7.1	60.5	68.0	70.0
Mean	—21.0	2.1	56.2	46.8

Moreover comparing the results of control sequence A, B, dissociated-II C and D, subject K. I. shows a smaller saving but subject N. Y. shows a greater saving.

From these results it may be inferred that (1) proactive inhibition of the interpolated learning upon the relearning might be prevented from the hypnotic dissociation when the latter is done in a different state from the former, and (2) it might suggest indirectly the existence of the hypnotic dissociation that the saving score is smaller when the original learning is done in waking state, and the relearning in hypnotic state. However, there would be the necessity for further and more precise experimentation because the available data are statistically insufficient.

TABLE 6

PERCENTAGE OF SAVING IN TRIALS AND ERRORS DURING REST-SEQUENCE

SUBJECT	TRIALS		ERRORS	
	Waking Rest	Hypnotic Rest	Waking Rest	Hypnotic Rest
H. M.	.....	73.3	.....	72.6
E. T.	65.4	.....	55.2	.....
A. Y.	66.7	93.6	46.0	90.9
T. M.	42.1	.....	53.8	.....
S. M.	.....	84.6	.....	95.7
Mean	54.1	84.0	52.2	88.7

Finally, looking at the results in Table 6, we may find the remarkable tendencies in the sequences that have rest periods corresponding to the interpolated learning periods. Table 6 shows

a greater saving in trials and errors for both waking rest sequence and hypnotic rest sequence.

Comparing the results of waking rest sequence and hypnotic rest sequence, we may find greater saving for the latter than for the former in agreement with Nagge's findings. These results may be accounted for by assumption that the interference effects of some activities occurred in the rest period on the original learning are interrupted by the hypnotic dissociation. There may be a close resemblance between the effect of rest in hypnosis and that in sleep and immobilization(2, 10, 13).

Moreover, comparison of the saving scores of dissociated-I-A (W-T-W) and those of rest sequence A (waking rest sequence) shows that the scores of dissociated I-A (W-T-W) and those of waking rest are nearly equal. It appears from these findings that the interpolated learning in hypnotic state has the same degree of effect as the rest spent in waking state, but (2) the effect is not equal to the rest in hypnotic state. In other words, it seems that retroactive inhibition effect is interrupted by hypnotic dissociation, but the degree of the interruption is not perfect and there remains some partial effect.

Discussion

The following may be inferred from this experiment, although it has some defects in methodology and experimental design.

Some parts of these experimental results support the dissociation hypothesis in hypnosis, in accordance with Nagge's results but in opposition to Mitchell's results. However, a part of these results suggests that the dissociation hypothesis implying "the complete functional independence between hypnotic and waking state" is inadequate, as Hull and his co-worker pointed out. For, if the two states are "completely" independent in their functions, the original learning in waking state should not be influenced despite the insertion of the interpolated learning. But the author's results, in accordance with Nagge's, show a significant difference between two conditions; dissociated-I-A (W-T-W) and rest sequence

A (Waking rest sequence, W-Wr-W). That is, the interpolated learning in hypnosis have left a little retroactive inhibition.

Then, the dissociation hypothesis may be supported in the sense that there is functional independence, although it is an incomplete one. In other words, the two states are functionally independent on one level, but another level as yet integrated.

Finally it is found that the dissociation hypothesis has a certain validity, though it should not imply "complete" functional independence between hypnosis and waking state. Especially, taking into consideration the available findings on post-hypnotic amnesia and hypnotic anesthesia, this hypothesis may have to be related to a level of awareness on which White and Shevach(17) and Weitzenhoffer(14) lay emphasis.

Furthermore, it may be useful for discovering new orientation of the dissociation hypothesis, as well as the model of "hypnotic stimulus shift effect" offered by Weitzenhoffer(15, 16) in relation to Nagge's results, to assume that hypnosis affects our level of awareness most strongly, and occasionally affects even the basic alloy of psycho-physiological activities, although the interaction of our level of awareness and the basic alloy which lies under our level of awareness and interacts very closely with it.

Summary

This experiment is an attempt to make experimental examination of the dissociation hypothesis in hypnosis, using the phenomenon of retroactive inhibition.

In general, the first material was learned in waking state, and the second material was learned in hypnotic state.

The summary of the experimental results is as follows:

(1) The interpolated learning in a different state from the original learning showed greater saving scores than both in the same states.

(2) The interpolated learning in hypnosis showed saving scores which are nearly close to the scores attained when the rest was given in waking state instead of the interpolated learning. They, however, were smaller than the scores attained when the rest was given in hypnosis.

It was considered that these results suggest incompleteness of functional independence between hypnosis and waking state, and as yet remains of some close relation on the basic alloy under the level of awareness.

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FURTHER STUDIES IN THE USE OF HYPNOSIS AS A METHOD OF ANESTHESIA

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For centuries a variety of reports has been made in the literature either praising, endorsing, condoning, or rejecting hypnosis as a method of anesthesia. Some writers contend that hypnosis is the ideal method of anesthesia for practically all types of cases. On the other hand there are others who reject hypnoanesthesia as unsatisfactory under any conditions. Fortunately both of these groups are made up of comparatively few persons. The medical psychologist engaged in research concerning hypnoanesthesia is merely attempting to investigate further one of the oldest methods in the field of psychology. In the more recent past another impetus has occurred in the use of hypnosis as a method of anesthesia. Because of the mutual interests of some anesthesiologists, psychologists, and psychiatrists in hypnoanesthesia, cooperative research has been fostered between these specialists(1, 2, 3, 4, 5, 6, 7, 8, 9, 10). Neither anesthesiologists, psychologists, nor psychiatrists would attempt to prove that hypnoanesthesia is the superior method. Researchers are applying hypnoanesthesia to those kinds of patient problems in which hypnosis is the logical choice of anesthesia. It is neither likely nor plausible in the field of anesthesia that any one single method will prove best for all patients. Individual differences must always be considered. There is no longer any doubt that hypnoanesthesia may be the preferred method in certain types of cases. However, in a series of recent articles the attitude of conservativism in the use of hypnoanesthesia has been expressed(1, 2, 5).

There are scores of papers in the literature describing the effects of hypnoanesthesia. However, the work first reported by Sears(14) is probably most unique and void of subjective evaluation on the part of the hypnotic operator. Sears demonstrated that hypnotized subjects manifested less reaction to painful stimuli than did a non-hypnotized control group. Physiological signs such as psychogalvanic responses, facial flinching and pulse rate were com-

pared between the experimental and control groups. A significant change between the two groups existed, with the hypnotized group showing significantly less reaction to the pain than the non-hypnotized group. However, the hypnoanesthesia produced did not approach the state one would have obtained if the patient had been anesthetized with chemical anesthesia.

In a series of more recent articles (1, 2) in which hypnoanesthesia has been employed with seriously ill patients many interesting observations have been reported concerning physiological reaction to pain. During surgery, for example, some hypnotic operators have reported few if any changes in the patient's blood pressure, respiration, pulse rate, and other signs usually associated with pain. Although controlled studies have not been done we feel that seriously ill patients may enter deep hypnotic states much more readily and frequently than persons who are not ill. In our experience patients who are seriously ill, especially if they have experienced much pain, have a tremendous desire to cooperate and have a high degree of motivation towards the hypnotic method, which are prerequisites for a somnambulistic state. This is especially true when patients realize that hypnoanesthesia might serve as a life-saving method. When a patient enters the deep stages of hypnoanesthesia he will physiologically perceive less pain than the person in a lighter trance. Recent evidence reported also indicates that in the deep trance states cortical changes might occur possibly related to those changes found in chemical anesthesia (1).

In attempting to make a decision concerning the use of hypnoanesthesia, the medical psychologist's opinion will often be sought. The following factors should be considered: Hypnoanesthesia is originally time consuming. Exceptions can be found, however, especially if the patients prove to be excellent somnambulists; some patients can be hypnotized but not to a depth of hypnoanesthesia required for long surgical procedures; there are also certain individuals with personality manifestations in which the use of hypnosis would be contraindicated.

When hypnoanesthesia is employed several advantages favoring both the patient and the physician may be derived. Once the patient has become hypnoanesthetized it is possible through the use

of post-hypnotic suggestion to reinduce the patient in a very short time. During the surgery the rapport can be transferred, for example, to a neuro-surgeon in order that he might do a neurological examination requiring perception and accurate response from the patient. To carry out the same exacting neurological examination when chemical anesthesia is employed presents the obvious difficulties of awakening the patient at exactly the correct moment, having his understanding and cooperation, and obtaining a reliable response. Post-operative sedation may either be reduced or totally eliminated by the substitution of post-hypnotic suggestion controlling pain.

The following cases exemplify the advantages of using hypnosis when it was considered the choice of anesthesia by physicians in charge of the case.

Case 1. A 30-year old man had thermal burns covering 45% of his body surface. During the initial phase of treatment he required numerous dressing changes and debridements. Because of the extreme pain involved in the manipulation of his extremities it became necessary to administer a general anesthetic during these surgical procedures. He became progressively fearful of the frequent anesthetics and was becoming generally uncooperative, hostile, withdrawn, and negativistic. It was decided that the patient might benefit both physiologically and psychologically if hypnoanesthesia were used during dressing changes and debridement. He was an excellent subject capable of entering a deep somnambulistic state. It became possible to change the patient's bandages and extensively debride the tissues under hypnoanesthesia without any perception of pain prior to, during, or following the procedures. There was a definite change of attitude on the part of the patient. He became cooperative, friendly, and apparently unafraid of his hospitalization. Hypnoanesthesia was used as the anesthetic during the next surgery. The patient's burns were debrided extensively, and a split-thickness skin graft was taken from the left leg. Prior to the completion of surgery he was given the post-hypnotic suggestion that he would perceive no pain in the donor area or the area of debridement. Two days later pain and touch sensations were restored by hypnotic suggestion. The patient's recovery was rapid and without complication.

Case 2. A 44-year old married woman was hospitalized with a diagnosis of active and extensive pulmonary tuberculosis. This patient also had a history of being treated for tubercular meningitis. During the course of her present illness she began complaining of abdominal cramping and excessively long menses. Gynecological examination was done and the results indicated cancer of the cervix. It was necessary to surgically treat the patient as soon as possible through the implantation of radium at the site of the cancer. Because of her anxiety concerning her respiratory condition coupled with her past history, she was fearful of an anesthetic procedure. Hypnoanesthesia was suggested and fully explained to the patient. After rapport was established hypnosis was attempted. After two hours of conditioning the patient was able to enter a somnambulistic depth of hypnosis. During the surgery the cervical radium implantation was accomplished without difficulty. She did not complain of any pain or discomfort and was able to sleep approximately three hours post-operatively without any sedation. Her recovery from surgery was uneventful and free of complication.

Case 3. The patient was a 23-year old female who had previously undergone surgery for resection of an acoustic neuroma. Post-operatively there was a residual facial palsy which persisted. A second phase of the surgery was being done in order to effect restoration of the damaged facial nerve. The surgeon wished to operate under local anesthesia so the patient could report changed sensations in the affected areas. However, the patient had previously demonstrated a low pain threshold, and the surgeon anticipated that she might react adversely to pain inadvertently produced during a local anesthesia procedure. It was evident that the patient was quite anxious concerning the surgery and that her cooperation would be necessary during certain phases of the procedure. When the surgeon suggested the possibility of using hypnosis she was initially doubtful and apprehensive, but she was willing to cooperate "and try her best." She was conditioned for the operation by being hypnotized twice prior to surgery. During the operation she did not move or complain of any pain or discomfort. She answered the surgeon readily and without doubt or hesitation when questioned about sensations perceived. The procedure was completed in five hours

and was accomplished without apprehension or fear or any untoward reaction on the part of the patient. She responded to the post-hypnotic suggestion to sleep for about four hours post-operatively. Her recovery was uneventful and free of complications.

The purpose of this paper is to present some of the problems involved in the use of hypnoanesthesia. The anesthesiologist and the surgeon usually decide on the type of anesthesia that a patient will receive. When, however, hypnoanesthesia is being considered the medical psychologist is frequently called either for consultation or for active participation. There are many problems to be considered, some of which have been presented in this paper. Although this conclusion lacks controlled experimental verification our experience indicates that seriously ill patients are capable of entering deep states of hypnosis more readily than persons who are not ill. If this assumption can be experimentally validated it is hypothesized that the sick person has a greater degree of motivation which probably would account for the extremely deep hypnotic states that we have observed in these patients. If the patient is insufficiently motivated or highly resistant to the method, hypnosis will probably be unsuccessful. Also if his illness is of such severity that he has a critically depressed metabolic status, he may not be able to cooperate to the required degree.

Hypnosis, like any other method of anesthesia, should be employed in terms of the patient's particular status. At the present time it appears that there are several problems in which hypnosis is an acceptable and perhaps preferable method of anesthesia. Some of these problems include cases in which chemical analgesics and depressants are contraindicated or dangerous because of respiratory or cardiac disease. It may be indicated with patients who have demonstrated sensitivity to certain local anesthetics. Hypnoanesthesia may be used in cases in which repeated chemical anesthesia tends to have a debilitating effect on the patient with an already disturbed physiology, such as patients with thermal injuries who require multiple repeated debridements and dressings. Hypnosis may also obviate the debilitating effects of prolonged chemical anesthesia.

The disadvantages in using hypnoanesthesia include the fact that some persons cannot be hypnotized. Furthermore, some persons who can be hypnotized are not capable of entering the deep hypnotic state necessary for anesthesia over a long period of time. It may be originally time consuming. It requires special skill and training on the part of the operator, and there are also risks to be considered in hypnotizing patients who have psychological problems.

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BOOK REVIEW

SHAW, S. IRWIN. Clinical Application of Hypnosis in Dentistry. Philadelphia: Saunders, 1958.

A sincere effort has been made by Dr. Shaw to consolidate a wide variety of mechanistically oriented hypnotic technics which are applicable to dental practice. The clinical methods discussed are given in systematic detail, accompanied by simple line drawings which are meant to demonstrate the situation. The technics themselves are presented as if they are to be learned by rote, and when learned constitute an ability to deal with the problems of clinical hypnosis.

It is unfortunate at this time, when serious workers are making scientific progress in the intergration of hypnosis within the framework of psychodynamics, that there are still workers and teachers who believe that it is a subject which can be taught in simple mechanical terms, while paying lip-service to psychological small-talk. This type of rigidly structured technical instruction is of questionable value, since it is dangerous for the neophyte and useless to the advanced worker. Although the technics and methods described are generally accepted and in common use in the fields of medicine and dentistry, the oversimplification renders them meaningless.

There are many aspects of dentistry which require manuals of laboratory instruction which must be followed in exact detail. It is, however, naive to apply this laboratory methodology to the problems of human motivation and response. It can only result in disillusion, disappointment, and possibly disaster.

In the introduction, Andre Weitzenhoffer hopefully suggests that this book be used "as a simple working manual . . . a handbook for routine procedures." This is surprising in view of Weitzenhoffer's statement in 1955 that "effective induction of hypnosis is more than the mechanical application of certain agents and technics." He has said further that "hypnosis is a psychological process and the effective hypnotist is an effective psychologist."

Probably most startling to the experienced reader is the appendix which offers what is described in the introduction as "the

framework of psychodynamic behavior." Within the 22 pages which constitute the appendix, the reader is exposed to a series of meaningless platitudes which encompass the forces of human behavior. It is difficult to understand why an experienced worker in the field of hypnosis should attempt this type of oversimplification.

It is to be sincerely hoped that Dr. Shaw's book will not be considered dentistry's attitude toward hypnosis, for that would certainly be an erroneous impression.

ARTHUR H. WALD, D.D.S.

BOOK REVIEW

KLINE, M. V. Freud and Hypnosis. The Interaction of Psychodynamics and Hypnosis. New York: Julian Press, Inc. and The Institute for Research in Hypnosis Publication Society, 1958, 207 pp. $4.00.

In the years to come, in the light of history, this monograph may well be considered as one of the best in its field to have been published in the past half century. Written by one of the foremost researchers and clinicians in the field of hypnosis, it brings together in an easily understood and succinct way the distillate of contemporary thought in hypnosis as compared to Freud's published views on the matter.

One chapter is devoted to direct quotations from the works of or about Sigmund Freud, with respect to his reasons for abandoning hypnosis. The need to deal with this problem developed as part of the more general problem of the reasons for the continual rejection of hypnosis by the "formal" psychoanalytic movement. In reviewing Freud's comments, it developed that he had two major types of motives for rejecting hypnosis. One was subjective and, as the presumably self-determined wish of an individual, can be accepted on those grounds alone. The second represents Freud's "objective" or scientific reasons for giving up hypnosis and are essentially the reasons still held by some psychoanalytic groups.

In the light of contemporary research and scientific knowledge about hypnosis, these objective reasons, while perhaps valid in the days of Freud (and even then there may be some question), no longer appear valid in most respects. To persist in using them reflects a lack of direct experience with hypnosis, either didactically or experimentally. It likewise seems to involve a denial that hypnosis as an area of serious scientific study has changed from the days of Freud.

On the basis of Freud's objective refutation of hypnosis, it would seem that he thought of hypnosis more as a definitive therapy than as a complex tool or modality. He was critical of suggestive therapy and linked hypnosis with this approach, in spite of the fact that Sidis, Prince and Janet had demonstrated that hypnosis could

be utilized for intensive studies of personality dynamisms, the mechanisms of which were revealed and not covered over by hypnosis. To reject hypnosis in psychotherapy today on the "suggestion basis" is to expose one's naiveté on the subject and reliance on authority in the absence of personal experience.

Contemporary research in clinical and experimental hypnosis refutes all of Freud's objective reasons for abandoning hypnosis. Many pertinent studies substantiate this statement. Dr. Kline draws seven general though salient conclusions which are the essence of the problem. With the advances in clinical and experimental hypnosis, it has become apparent that the utilization of hypnosis in these fields is a specialized area of activity, a skill that demands a considerable investment of time and effort to acquire. Part of the resistance to hypnosis involves a wish to deny this fact. Thus we find, all too often, that the criticism leveled against hypnosis comes from individuals relatively untrained and naive in this modality.

To a great extent the figure of Sigmund Freud and some of his observations have been built into a legend of hypnotic omnipotence and, on the basis of his observations and conclusions, hypnosis has been dismissed as an obstructionist element in psychodynamics to be excluded from serious consideration. In this lies one of the greatest distortions of scientific truth in modern times.

Dr. Kline devotes the balance of the monograph to a positive and constructive demonstration of the nature of hypnosis, to description of observations and experiments in the hypnotic state and with hypnotic relationships. He demonstrates how the elements of psychoanalytic psychology and of contemporary psychodynamics are an integral component of hypnotic phenomena and behavior and what role hypnosis has and should continue to have in the behavioral sciences. For, in this respect, it will be clearly seen that the theoretical and conceptual developments in psychoanalysis are not in contradiction to but are congruent with the dynamics of hypnotic behavior.

Since the American Medical Association, at its last annual meeting, found that there is no longer any need to question the validity of hypnotic phenomena nor question the existence of hyp-

nosis itself, it paves the way for a tremendous advance in its study and use by all the healing professions. The monograph under review will do much to place contemporary hypnosis in proper perspective. Our own perceptual blocks, our own misconceptions, and our own doubts have prevented more clinicians and research investigators in psychology, medicine and the allied behavioral sciences from working with hypnosis freely and unfettered by their own emotional involvements and conflicting attitudes.

In this reviewer's considered opinion, with forty years experience in reading literature on hypnosis, Dr. Kline's volume should do more to clarify the issues involved and to remove the blocks that have obstructed the development and use of hypnosis in the past than any other book he has read. "Freud and Hypnosis" should become one of the most valuable volumes in the library of any worker or student of hypnosis or of any of the behavioral sciences.

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In Memoriam

DR. S. KOSTER

On February 2nd Dr. S. Koster, pioneer in the field of hypnosis passed away in Amsterdam.

Dr. Koster was born October 6th, 1894 and has practised about 30 years as a neuro-psychiatrist in the Dutch capital. From his hand many important publications appeared, mainly in the field of hypnosis. He was one of the first to propagate the instruction of hypnosis at the university. Many courses for physicians in this field have been given by him.

From his 14th year onward he wanted to become a psychiatrist. His chief work is the Textbook of Hypnosis, of which the 4th edition appeared shortly before his death.

Dr. Koster was especially experienced in the treatment of enuresis with the aid of hypnosis and much has been published by him about this treatment. His many papers mainly appeared in Dutch journals. Dr. Koster, who was a member of the "British Society of Medical Hypnotism" and a Honorary Member of the "Society for Clinical and Experimental Hypnosis" wrote several articles for the "British Journal of Medical Hypnotism" and the "Journal of Clinical and Experimental Hypnosis". An industrious and fruitful life came to an end. May he rest in peace.

B. Stokvis, M.D.
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